Elementary Arithmetic. By C. Pendlebury, M.A., F.R.A.S., and W. S. Beard, F.R.G.S. (London: George Bell and Sons, 1890.)

IN a book on elementary arithmetic it is necessary that there should be throughout a good and well graduated series of examples. The authors of the present volume have got together a large number of examples and problems for written work, and in addition they have arranged numerous sets for use in oral teaching form, a very important feature in an elementary work of this kind. The explanatory matter is written in intelligible and simple language, and great attention has been paid to the treatment of the money rules and the more important weights and measures.

This book is intended to serve as an introduction to the one on "Arithmetic for Schools," and the examples have been arranged so that they are all different from those given in the advanced work.

LETTERS TO THE EDITOR.

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British Association Procedure.

IT has been to me a matter of surprise that a letter by Prof. O. Lodge published in these columns on October 17, 1889, did not elicit other similar communications, as the views he enunciated are undoubtedly those of a not inconsiderable number of active members of the British Association. Prof. Lodge pointed out that the British Association week undoes the benefit of the previous holiday, mainly because the conditions under which the work of the Sections is carried on are prejudicial to health. This I know from considerable personal experience to be the case; and in this and previous years I have had the remark addressed to me: "Surely you are not going to *that* British Association meeting to make yourself ill again."

Prof. Lodge suggested that the Sections should sit from 10 a.m. till 1 or 1.30 p.m., and that the Sectional Committees should meet afterwards. Such an alteration would doubtless mitigate the evils inseparable from the present system, and it is to be hoped that a determined effort will be made at the ensuing meeting to promote its adoption. A recommendation somewhat to this effect was, I believe, made to the Council by at least one of the Sections at the Birmingham meeting, but nothing came of it. This is, perhaps, not surprising; indeed, it is a question whether anything ever does come of recommendations to the Council—so some say.

Machinery devised 50 odd years ago is no longer capable of satisfactorily coping with modern requirements. We do not go to British Association meetings to sit for hours together to hear papers read such as we have listened to *ad nauseam* during the previous sessions—our main object is to meet and exchange views; but everything seems to be done to prevent rather than to promote this. Far fewer papers should be read; far more care should be devoted to the selection of papers; much more should be done to encourage discussion, especially between Sections; and ample time and opportunity should be given for conversation.

The Sectional Committees are absurdly unwieldy bodies, and in the case of some Sections, practically comprise the entire Section: an appeal was made to the Council by my Section to put a stop to a practice which enables all the nobodies to become members of the Sectional Committee, but without result: I believe we were told that we could do as we liked. Had this been the case, we should scarcely have troubled the Council. The Sectional officers, with at most half a dozen other members, would form a far more useful Committee than any larger number; but if it be thought otherwise, let the whole Section sit as a Committee.

Lastly, a word may be said as to the date of meeting. Could any time be more unsuitable than the beginning of September? Most of those who are engaged in advancing science are then in the very middle of their holiday, and can attend only at grave personal inconvenience. HENRY E. ARMSTRONG.

The Mode of Observing the Phenomena of Earthquakes.

THE publication of Mr. Davison's paper "On the Study of Earthquakes in Great Britain," in NATURE of the 7th inst. (p. 346) furnishes me with an opportunity of making a few *remarks*, followed by a *suggestion* as to the mode of recording the effects of seismic disturbances of the earth's crust on the apparent change of position, especially of vertical objects, in the field of vision of the observer.

Remarks.—It will, I think, be admitted that the descriptions of the alleged rocking to and fro of walls, towers, and chimneys, may not unfrequently convey an exaggerated idea of what really takes place; and, probably, the same is true of the narratives of personal experiences of reeling or rolling movements on the part of the narrator. I refer, of course, to the alleged *extent* of these movements, for no one can doubt their actual *occurrence* as the result of a *tremblement de la terre.* Such composite structures as walls, towers, and chimneys have a real flexibility and elasticity, as is shown, for example, by the opening and shutting up of cracks and fissures in their substance. But the extremely vivid accounts which we read of the swaying to and fro of solid buildings, as witnessed by persons in the upright position, and by others who are recumbent, suggest at least that some of these recorded disturbances of position in external objects may be more apparent than real, and may depend on some sudden uncontrollable movement of the head, and therefore of the optic axes of the observer's eyes.

It is well known that, when the head is moved swiftly to one side and back again to a vertical position, upright objects seen in front, appear to shift from their vertical position in an opposite direction, and then back again. It is not here needful to explain scientifically this very obvious phenomenon. A similar apparent displacement of objects, though in a vertical direction, occurs when the head is nodded backwards and forwards. Movements of the head in intermediate directions produce intermediate effects; whilst rotatory movements of the head give rise to corresponding though mixed kinds of disturbance of objects in the field of vision. Lastly, if the observer is in a horizontal position, as in bed, for example, a sudden rolling over of the head to one side and back again produces like phenomena.

Now such disturbing movements of the optic axes must frequently occur in the case of persons suddenly subjected to the consequences of earthquake tremors, whether such persons are in a vertical or in a recumbent position; and it is difficult to understand how they should not *occasionally* seem to exaggerate the apparent effects of the disturbance of the earth's surface and the objects planted upon it. The equilibrium of the observer's head is suddenly disturbed in a given direction, and an opposite involuntary movement instantly occurs in order to restore the previous condition of things. Granting, then, the *objective* reality of the swaying movements of vertical structures during earthquakes, there seems to me to be reason to think that the effect of these is occasionally enhanced, and their record influenced by the *subjective* impressions due to movements occurring in the observer's own optical apparatus.

Suggestion.—Supposing this view to be correct (though I can furnish no direct proof of its truth from earthquake records), it appears to me that the *suggestion* of which I spoke at the commencement of this letter would be a useful addition to paragraph (*a*) of Section 2, Division A, of Mr. Davison's paper (p. 348), which relates generally to the "situation of the observer." This suggestion is that it should always be noted and stated towards which *point of the compass* the observer's face was directed at the moment of each observation, concerning the deflection of upright buildings, rocks, and so forth, especially of their lateral deflections. For it is obvious that if a sufficient number of such observations were recorded, it ought to happen, on my hypothesis, that persons who looked *across* the earth-waves would be moved up and down, and thus would have the vertical movements only of objects in front of them exaggerated, whilst persons who looked along the waves would be swayed sideways by the undulations of the soil, and would therefore have the extent of the lateral movements apparently increased. Under the former condition, the "little hills" might appear to dance; in the latter case, cliffs, towers, walls, and chimneys might seem to sway inordinately from side to side.

Many such observations, duly recording the variations in the apparent *extent* of the movements noted, together with the *positions* of the observer as regards the compass, would, when

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compared with the ascertained direction of the earth-waves, confirm or upset my general supposition. But, if this were found to be correct, such observations would furthermore constitute, even in the absence of special seismic instruments, a certain amount of evidence as to the actual direction of the earth-waves on any particular occasion.

Similarly, a record of the exact direction of recumbent observers in regard to the points of the compass, might, when compared with their respective descriptions of the movements of objects about them, serve a similar purpose.

Man himself would thus to a certain extent—that is, as regards the local direction of the earth-waves—be his own seismometer. Possibly, some evidence on this subject might even now be obtained by comparing the descriptions of the appearances with the ascertained directions of the outlook of different observers. JOHN MARSHALL.

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On a Problem in Practical Geometry.

IN treatises on practical geometry rules are given by which an arc and its chord or an arc and its tangent may be divided proportionally, but they leave an error which is often too great.

By the following method the points of division move step by step towards their required positions until errors are of less than any assigned amount.

Let GMH be the chord (Fig. 1), M its middle point, AOB the perpendicular diameter. In AOB produced take a series of points $B'B''B'''$..., determined thus: $BB' = BG$, $B'B'' = B'G$... Then evidently circles with centres at $BB'B''$..., passing through G, form a series of which each has on its circumference the centre of the succeeding one. These circles cut the line AM in a series of points $A'A''$..., and the arcs GAH, $GA'H$, $GA''H$... get rapidly nearer the straight line GH. Any point C on the given arc GAH may now move to its destination γ on the line GH by stepping up to each circle in the direction of its centre; along a path $CC'C''$..., made up of CC' tending to B, $C'C''$ tending to B', and so on.

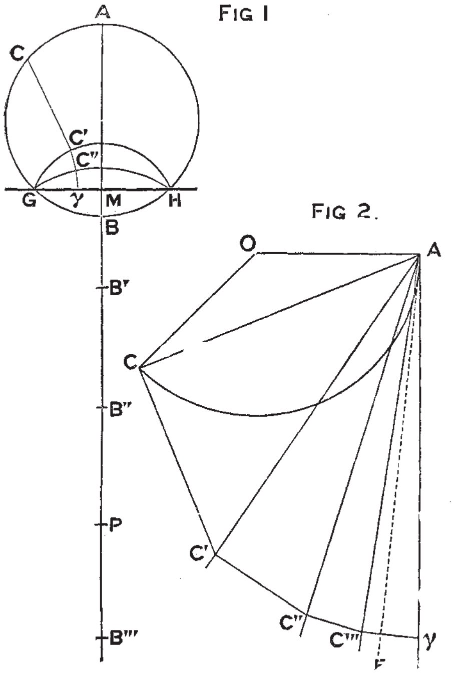
FIG I

FIG 2.

That all the arcs which this path crosses and the chord to which it tends are divided proportionally at the points $CC'C''$... γ follows from the almost obvious theorem that if the centre of one circle is on the circumference of another, lines drawn from that centre intercept arcs of the circles having a constant ratio.

If the circles become inconveniently large before the required approximation is reached, we may use the following: $C'C''$... are the centres of the circles inscribed in GCH, $GC'H$..., which are easy to construct. Also it may be noticed that the angle made with AO by each of the parts of $CC'C''$... is half that made by the preceding part, and the process may be brought to an end at any stage with progressive accuracy by making the last angle one-third instead of half of the preceding one.

In Fig. 1 the process is closed after the second stage by drawing the last line ($C''\gamma$) not towards B'', but towards the middle point P of $B''B'''$. This may be done as soon as the last arc $GC''H$ comes to be less than a quadrant.

When the chord becomes the tangent AT at A, the points $AA'A''$... coincide, all the circles have AT touching them at A, the radius of each is half that of the succeeding one, the arcs intercepted AC, AC', AC''... are equal, and so we get in the limit $A\gamma$, the length of the arc AC laid out on the tangent.

But in Fig. 2 an alternative construction is shown. Bisect TAC by AC', TAC' by AC'', and so on. Draw CC', $C'C''$... at right angles to AC, AC'... The process is shown closed after the third stage by drawing $C'''\gamma$ at right angles, not to AC''', but to a line AF such that $C'''AF$ is one-third of $C'''AT$. In the result $A\gamma$ is equal to arc AC.

JOHN BRIDGE.

Caught by a Cockle.

I HAVE often intended writing to you describing a curious occurrence which I witnessed on the coast of Queensland in September 1889, but I have as often forgotten to do so when the opportunity came. While out shooting, along a sandy beach, I noticed a small muddy patch just covered by the rising tide. In this I observed a bird, a sand-piper, which seemed to be striving in vain to rise. I could not think how the bird had become caught, but on coming up to it I found that one claw of one foot was firmly held by a large cockle (about 1½ in. by 2 in.). Of course the bird would have been drowned eventually (though the benefit to be derived by the cockle seems rather problematical); and though it seemed to be aware of its danger, yet it had made no attempt to free itself by trying to bite through the claw, as one sometimes reads of animals doing when caught in traps. As I believe this is rather an uncommon incident, I must make that my excuse for troubling you. D. MCNABB.

H.M.S. *Dart*, New Hebrides, July 3.

ON STELLAR VARIABILITY.

ON the hypothesis of the meteoritic origin of the various orders of cosmical bodies there is a grand and orderly variation, both in light and colour, in the case of every undisturbed swarm during its condensation from its most nebulous condition to that of a cool dark globe.

As by virtue of the ordinary evolutionary process an *undisturbed* swarm successively passes through the changes the results of which define the various groups, the light will wax through Groups I. to IV., and then wane till it is finally extinguished; at the same time the colour sequences will be successively passed through. But with such a variability as this, compared with the period of which our *annus magnus* is but a point of time, we have now nothing to do. We have to deal really with *disturbed* swarms or with double or multiple swarms through their various stages of condensation.

Let us take the purely disturbed swarms first. Imagine a nebula, sparse, and therefore so dim as hardly to be visible at all. Then, further, imagine the appulse of another, or the approach of a meteoritic stream. We shall have the condition which must bring about increased luminosity; the outburst may be short or sudden; the greater luminosity may last a short or a long time; the dying down of the light may be fast or slow. In that we shall have the possibilities of new and dying stars.

If the spectrum of the light produced by this clashing be observed, we may not have precisely the same phenomenon as that observed in the various groups defining

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